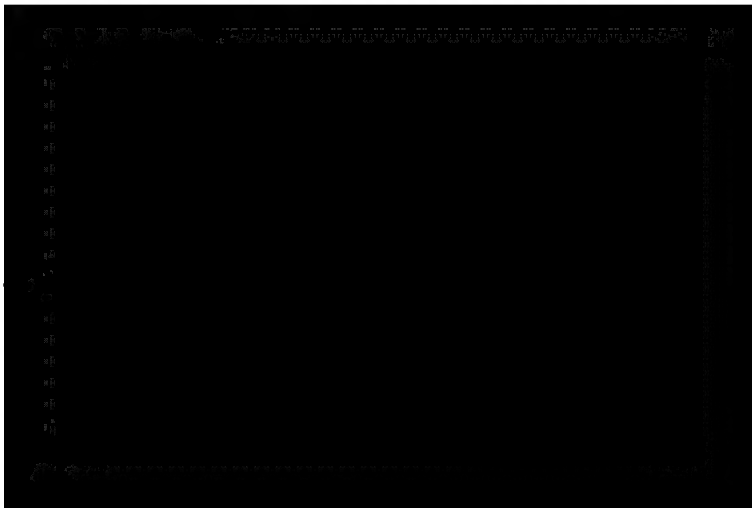


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PRECOGNITION IN MICE AND JIRDS¹

By W. J. LEVY, JR., and ANITA McRAE²

ABSTRACT: In a previous experiment which was carried out to confirm the research by Duval and Montredon with mice, the overall results were not sufficiently high to be considered a replication. The research presented here was a second, longer attempt at confirmation. Essentially, the experiment tested the ability of the animals (mice and jirds) to use their precognition to avoid jumping into whichever side of their cage would give them an electric shock on the forthcoming trial. A pilot and a confirmation series were carried out and both were significant. The confirmation series was interrupted part way through to test the authors' impression that the animals' scoring tended to decline when they were kept too long in a static environment. The results of four occasions were compared with regard to scoring before and after the cages had been cleaned and rearranged. The scores were found to be below chance in the "before" condition and above chance in the "after" condition, with a significant difference between the two.—Ed.

A year after Duval and Montredon published results of their precognition work with mice (1), Levy, Mayo, André, and McRae carried out an attempted confirmation (2). The French work had presented evidence that mice could use their ESP ability to avoid being in whichever side of their cage would be chosen to be shocked on the forthcoming trial. Although the work by Levy, Mayo, et al. showed certain significant internal effects having to do with the animals' behavior during the test situation, the overall results were not sufficient to be regarded as an independent replication of the French work. The purpose of the research now to be reported was to complete a confirmation of the French work and to substantiate the first attempt to repeat it.

APPARATUS

As in the previous experiment, the apparatus had six components: (A) a shock box; (B) a position indicator; (C) a random

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² The authors wish to acknowledge the valuable advice and encouragement of Drs. J. B. Rhine and R. L. Morris. They wish also to thank: Dr. Helmut Schmidt for his work in designing the logic circuit and the random generator; Mr. Peter Ensign, for constructing them; and Mr. Allen Mayo, Jr., for his general advice on design.

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target generator; (D) a logic circuit; (E) a shock control; and (F) a recording apparatus.

A. The shock box was a bottomless lucite box or cage measuring $6\frac{1}{2}'' \times 6\frac{1}{8}'' \times 6''$. There was a $6\frac{1}{2}''$ covered hole in the top through which the animal could be inserted and removed. A low barrier $1\frac{1}{8}''$ high and $\frac{1}{4}''$ thick divided the cage into two halves, each wired independently. The cage rested on a wire grid with two electrically separate halves corresponding to the two halves of the box. Each half had alternate wires attached to the voltage supply and ground. The wires were spaced $\frac{1}{8}''$ apart in the first part of the experiment. Later they were placed $1/16''$ apart. Beneath the box during the latter part of the experiment was a bed of CaCl_2 and cedar shavings. The cage was covered by an insulating box 1" thick, which had a 2" hole in one side for the light source to shine through and a $6\frac{1}{2}''$ square hole in the top for transferring the animals.

B. Part of the position-indicating apparatus was a set of mirrors cemented on the sides of the cage at right angles to the barrier. These, together with a light source and photoresistor in each half indicated the animal's position. If the light beam was not interrupted by the animal, it reached the photoresistor and the animal was recorded as present; if it was interrupted, the beam did not illuminate the photoresistor and the animal was recorded as absent.

C. The random target generator was an electronic device which randomly picked side A or B as the target for each trial. This generator is the same type as that described by Schmidt in 1970 (4). There was one opportunity for a target selection, and therefore one trial of five seconds duration each minute.

D. The logic circuit was designed to perform the same tasks described in more detail in the first paper by Levy et al. (2). It sent a five-second shock to the side of the cage the animal was recorded as being in, if that side and the target matched. If they did not match, no shock was sent. If the animal was recorded in both sides or neither—for example if he was on the barrier—a mistrial was declared, no target was chosen, and no shock was sent. With this system the animal seldom jumped into a shock since he had to jump two or more times in a five-second interval to do so.

E. The shock-control device was a Harvard Apparatus Co. animal stimulator, giving a shock of approximately 7 μ amps, at a

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frequency of 120 per second. The shock was individually adjusted for each animal at the start of each run at a level just sufficient to make him respond to three or four shocks in a row by moving into the opposite side. It was not set high enough to make him consistently change sides during a shock throughout the run. The shock was switched on to either side by a special switching box.

F. The recording device was an Esterline Angus ten-pen recorder. (See Figure 1.) The chart paper rolls were numbered before the experiment and were used in that order. The paper was never cut, so that a continuous record of the work is available. Six channels were used: one for each of the two sides of the cage to record

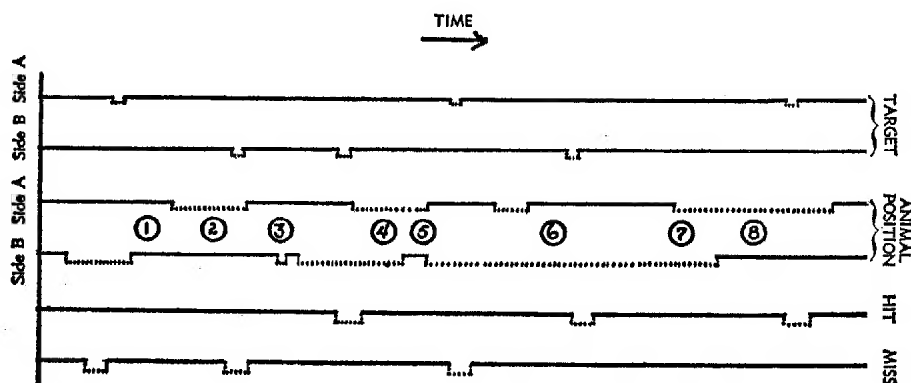


FIG. 1. Facsimile of chart showing method of determining the animal's position. The solid line shows when the pen was centered (animal present in the corresponding side of the cage). The dotted line shows when the pen was off center (animal *not* in that side of the cage). If the animal crossed from side A to side B, both pens might center (1) while he was crossing, which would indicate that he was present in both sides for a certain time.

When the pen for side A went off center (2) the animal was considered to be in side B. If the pen for side A had not changed but side B again went off center (3), the animal was considered not to have left side A. Or if he was crossing from side A to side B and both pens were off center (4) he was not counted as in side B until that pen centered (5). If the pen for side B had not changed but had remained off center and the pen for side A again centered (6) he would be considered not to have left side A.

Should he go from side A to the barrier and cause a mistrial so that neither pen centered (7), then following that trial go to side B with pen B centered, this would be called a jump for the following trial (8).

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if that side had been chosen for a target; one for each side to show the animal's position; one to show if the trial was a hit (the animal had avoided shock); and a sixth to show if it was a miss (he had received a shock). In a mistrial no target was chosen and no hit-miss indication was given.

The criteria listed in Figure 1 show that a jump was counted only at its completion; also, for the jump to be completed, the pen in the side the animal left must not have been centered and the pen in the side he entered must have been centered. Once these criteria were fulfilled, he had to remain in that side for one division of the chart (approximately one second) or forfeit his position there. This method reduced a misleadingly large number of jumps that could have resulted from the animal's tail dipping into a side if he was on the barrier. These conditions were set up to establish a change of sides; but at the time of a trial the equipment judged position only at the instant of the trial.

One additional piece of recording equipment was a set of counters which totaled targets for side A and side B, and also totaled the hits and the misses.

The animals were 38 white mice (Swiss Webster) and 20 jirds (*Meriones unguiculatus*). The housing of all the animals and their interaction with the experimenter were given special care. The jirds were kept in groups of four to six in large cages 2' x 4' x 14"; the mice were housed singly or in pairs in 3/8" wire mesh cages 10" x 12" x 9". Both were given environments with wheels, bricks, styrofoam for tunneling, and other items. They were handled by the experimenter before each run and on each Saturday during the experiment. A 12-hour-light-12-hour-dark cycle was used, and a thermostat was set to maintain a 72-degree room temperature.

GENERAL PROCEDURE

The experiments were carried out in the summer of 1970 at the Institute for Parapsychology. Together they occupied three weeks of experimentation, and all data were then analyzed by computer. In addition, the run scores were checked by hand. Altogether there were an exploratory series, a pilot series, and a confirmatory series which was interrupted by three exploratory runs before its completion. The pilot was preset to be 100 runs or the number of runs necessary

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for 1,300 random-behavior³ trials. The confirmation was preset for 150 runs. Each run had 26 trials, but the first trial was not scored. It was used to determine only whether the first scored trial (trial number 2) had been preceded by a shock or nonshock. Before each of the two major experiments, the apparatus was checked for several runs to be sure it was functioning properly and that the shock was being sent to the correct side. In addition, it was briefly checked several times during each experiment. It was stipulated in advance that any event which interrupted the run, such as a power failure, equipment malfunction, etc., would scrub that run and that a new one would be started.

Each day of the experiment the experimenter took eight test animals (in a preset order) and brought them to the experimental room in a transfer cage. They were then put into eight individual holding cages in the testing room from which they were taken to be tested. Later the experimenter returned these and took eight more to complete the test day. Sometimes all eight in each group would be used, but fewer or more might be used on any given day. Usually 14-16 runs were done in a single day.⁴

Before each run the animal's number, the time, and date were recorded in an experimental book and on the record chart. The shock level was also recorded in the experimental book. The experimenter then put the run number on the chart, started the apparatus, and set a timer for one run (a minimum of 26 minutes). When the timer rang, the shock was turned off, the animal was removed, and the process was repeated with the next subject.

It was felt that, because of the significant results in distance experiments with human subjects, the mere separation of the experimenter from the test room would not reliably exclude him from affecting the results. It was therefore decided that he would remain in the room during the run. This saved time by allowing him to score the data for a day during that day and to punch it onto cards

³ As explained in the previous paper (2), generally speaking, random-behavior trials are those in which the animal jumps for no apparent reason (as contrasted with jumps he makes while being shocked).

⁴ The details of the housing and care of the experiment are too lengthy for complete detailing here; but as the environmental effects presented later will indicate, they may be of some importance. Therefore the author will supply more complete details on request.

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for the computer that same evening. The chart rolled off the recorder over a small table so that previous runs or the early part of a current run could be scored while a run was in progress. The scorer coded the data onto Fortran coding forms as follows: the animal's number was put on one line; on the next four lines the 26 trials were represented with seven numbers for each trial. The first four numbers were the number of jumps in each of four approximately 13-second intervals making up the 55 seconds between two five-second trials. (The first quarter of the intertrial period was slightly larger than the succeeding three, being about 15 seconds long.) A special screen overlay broke the trial into four quarters, and in any ambiguous case the jump was counted for the previous quarter. The fifth number was the target chosen: 1 for side A, 2 for side B, and 0 for mistrial. The sixth number was the position of the animal. 1, 2, or 0 were used as before. The seventh number was a record of whether the animal was on the opposite side of the cage after the five-second trial period. If he was on the opposite side of the cage, he had jumped an odd number of times during the trial, most frequently once, and a 1 was recorded. A zero indicated that he had not been on the opposite side after the trial. This was to allow the computer to simulate the mouse's jumping during the run as a partial double check of the scorer's record of target and position at each trial. Each experimenter checked the work of the other, and a third checker has gone over the work since that time.

RESULTS

For completeness, the two exploratory sequences are reported, as well as the two experimental series.

Once the equipment was in operation, a preliminary (exploratory) series of 12 runs was done in which there were 88 hits out of 162 random-behavior trials. The start of the pilot experiment was then declared; and in the following 105 runs there were 1,308 random-behavior trials (54.1% of the total trials) with 704 hits, a 53.7% scoring rate. (See Table 1.) This gives a $CR = 2.77$, $P = .005$ (two-tailed), a very encouraging level of scoring. Half a day later, a 150-run confirmation was begun, which finished with 1,721 random-behavior trials (45.9% of the total) and 909 hits, a 52.8%

Table 1
GENERAL RESULTS

	Targets	Hits	% Scoring	PQ	CR	P
Pilot	1,308	704	53.7	5.87	2.77	.005 ^a
Confirmation	1,721	909	52.8	3.18	2.34	.01 ^b

^aTwo-tailed.

^bOne-tailed.

scoring rate. This gives a $CR = 2.34$, $P = .01$ (one-tailed), which can be taken as a significant confirmation of the pilot experiment.

When the confirmatory series had been partly carried out, the experimenters began to notice a change in the animals' behavior. While formerly they had frequently been moving around in the box from side to side, they now stayed for long periods in a single side and worked at the small holes where the light beams entered or on the bars of the grid, actually fatiguing the wires to the point that on four occasions an animal was able to escape. They were also jumping out of the experimenter's hand on handling. Concurrently the scoring was decreasing.

Since no known changes had been made in the apparatus or animal care, it was hypothesized that the static nature of the environment might be responsible for the decline in scoring. The experimenters had observed that the high scoring of the preliminary test followed a period when the animals had been in small, crowded cages or had come from the supply house in a crowded shipping crate housing 24 of the animals. They had been put in the new cages only three days before the experiment. Also, as was mentioned in the previous paper (2, p. 8), the animals which sustained a scoring rate of 52.9% were moved into new cages twice during the period of the test.

On looking back in the present research, we found that there were three instances here also when the scores had increased after the animal cages alone or both animal cages and test cages had been cleaned and subsequently rearranged. Thus, because we felt this amounted to a change in conditions, we stopped the confirmatory experiment at run 103, a point preset at run 101. A senior staff member not in the experiment was present to witness both the

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cutting-off and later the resumption of the official testing. We completely cleaned and rearranged the animal cages and cleaned the test cage with alcohol. We also received 14 new mice which were used, with the jirds, for the remainder of the experiment. We started a short exploratory series then to see if the behavior of the animals reverted. They did stop trying to escape; they moved around the cage again as before, and were easily handled again. Therefore, after three exploratory runs (33 random trials with 21 hits), we resumed the original experiment at a point decided in advance and we soon began swabbing the test cage with alcohol frequently between runs to prevent the build-up of odors that had occurred before, on the possibility that this feature might have an effect on the animals' behavior and, consequently, on his scoring.

A comparison between the results on the runs just before and just after the cleaning and rearrangement of the cages shows an increase in scoring from the "before" to the "after" conditions. There were three cleanings, referred to above, which took place before the interrupting series. The five runs preceding each one were found to yield 119 random-behavior trials with 44 hits (37% scoring rate). (See Table 2.) The five runs following each cleaning gave a total of 152 trials with 87 hits (57.2% scoring). To be added to this are the five runs just preceding the interruption of the exploratory series and the five runs after it ended. When these are added in there is a total of 185 random-behavior trials with 71 hits (38.4% scoring) for the "before" condition and 221 trials with 131 hits (59% scoring) for the "after" condition. The $CR_d = 4.20$ ($P = .00002$) between the scoring before each break in the routine and the scoring after it. Since the authors knew about the low scoring just preceding the interrupting runs, since this aided in motivating them to make it, and since new mice were used after this last occasion, the CR_d is used only to indicate an effect to explore further.

DISCUSSION

This experiment was an effort to repeat and confirm both the French work and the authors' own first effort reported earlier. With two independent experiments significant at the .01 level, it can be regarded as a confirmation. Furthermore, some conditions which contribute to positive scoring by the mice, and a first possibility of

Table 2
COMPARISON OF SCORES BEFORE AND AFTER CAGES WERE CLEANED AND REARRANGED

Occasion	Five Runs Before Cleaning Cages						Five Runs After Cleaning Cages					
	No. Trials	No. Hits	% Hits	PQ	CR	P ^a	No. Trials	No. Hits	% Hits	PQ	CR	P ^a
First	36	14	38.9	49.1	-1.33	60	34	56.6	17.7	1.03
Second	30	11	36.67	71.0	-1.46	54	33	61.1	49.2	1.63
Third	53	19	35.8	80.0	-2.06	38	20	52.6	2.69	.32
Subtotal	119	44	37.0	67.8	-2.84	.004	152	87	57.2	20.88	1.78	.07
Fourth (Interrupting Series)	66	27	40.9	33.18	-1.48	69	44	63.7	75.33	2.28	.02
Total	185	71	38.4	54.0	-3.16	.0014	221	131	59.0	34.5	2.76	.006

CR_a ("Before" and "After") = 4.20; *P* = .00002 (two-tailed)

^aTwo-tailed.

below-chance scoring, are indicated. The possible effect of the environment, together with the analyses on trials following shock and nonshock and the number of jumps between trials, may give more clues to the operation of the psi shown here as well as behavioral indications of the conditions necessary to successfully continue the replication of this work. The stability of the environment may also help illuminate the decline effect. While the work so far has been primarily oriented to building a firmer basis for ESP in this experiment, future efforts will be directed to these latter ends.

A possible counterhypothesis to psi as a source of the results is that the apparatus may not have produced random targets. For this reason, the randomness of the generator was checked during the experiment whenever animals were not being tested. A total of 5,588 targets were chosen for side A and 5,709 for side B. This is a variation well within chance limits ($CR = 1.14$, $P = .13$, with a 50.6% scoring rate) and indicates a satisfactory degree of randomness.

The targets that were actually generated during the experiment were also checked for randomness. In the pilot, side A was chosen 1,226 times and side B was chosen 1,199 times. This distribution has a $CR = 0.55$, 50.5% scoring in the opposite direction from the previous check. In the confirmation, side A was again slightly favored with 1,658 targets, while there were 1,622 for side B; $CR = 0.62$, 50.5% scoring rate. These results are not consistent with the theory that a side bias in the machine corresponding with a side bias by the animal produced or substantially contributed to the scoring.

A further hypothesis could be advanced, however, that there was a pattern in the machine which the animals learned by nonpsi means. As shown in Table 1, the second experiment had a slightly lower scoring rate than the first, which does not support a long-term chronological learning effect. However, a better test is to examine the first versus the second half of the run. An increase in scoring here could favor this hypothesis. In order to check this possibility, the runs were split into the first 12 and the last 13 trials. There was an increase in scoring rate from 52.7% in the first half to 55.2% in the second. However, the opposite trend was found in the confirmation series (54.8% versus 50.14%). Although neither of these

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pairs of figures shows a significant difference, they run counter to the suggestion that the results could be due to learning by the animal.

This leaves the possibility that there was an unlearned pattern in the animals, also common to the machine, which was not produced by psi. This is unlikely, since three random generators of different design have been used in significant or marginally significant experiments (one by the French, one by Levy, Mayo, et al., and one used in the present research). In addition, the animals involve two different species, mice and jirds, with a large number of individuals in each group. Some of these animals were observed to move around a great deal, others very little. These individual differences do not seem to favor a pattern that would be common to enough animals to account for the scoring in this research, and there were no individual scorers who contributed a major portion of the significance. It may also be noted that each run is relatively short, 26 trials, limiting the number of repetitions possible for even simple patterns. The animals had a three- or four-day interval between runs for any individual, making long-term learning or expression of a pattern on the basis of 25 trials less favored.

The mistrial may also work against either the learning counterhypothesis or the innate pattern counterhypothesis. There were 200 of these events in the pilot and 520 in the confirmation. Coming, as they did, at irregular intervals, they would make it more difficult for the mouse to learn or express a pattern. They could also break up some machine rhythms since, in the event of a mistrial, no target is chosen and no shock is sent.

The question of psi on the part of the experimenter is still unanswered. The experimenter's presence in the room and his awareness of the run score soon after the run was finished could be considered to favor his involvement; but we cannot at present be any more confident in discounting him if he is not in the room. There is a possibility, as pointed out by Rhine (3), that there is an interaction between man and animal involving psi. One approach to the question of the experimenter's role is that he might be less likely to be involved if the scoring relates to the animal more than to him. Apropos of this point, the experimenters were not aware of the results of the detailed analyses during the experiment. They were not known until afterward, and it was too involved a procedure for

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either author to keep track of. If the scoring does relate more to the animal's situation than the experimenter's, this may help make the experimenter less suspect.

The next paper will present a confirmation of the shock-non-shock, high-jump-low-jump analyses from this work, and some additional breakdowns. The pilot and confirmation reported here, with scoring rates similar to the first experiment reported, indicate that the weakness of that first experiment may have been that it was too short. The three experiments, together with more detailed analyses, will make a promising start with which to pursue this evidence of psi in animals.

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AN INTERVIEW WITH DR. RÉMY CHAUVIN

By J. B. RHINE

Dr. Rémy Chauvin, professor of animal sociology at the Sorbonne in Paris, was the principal speaker at a luncheon on January 2, 1971, during the Winter Review Meeting of the Institute for Parapsychology. The address was in the form of an interview in which Dr. J. B. Rhine presented to Dr. Chauvin a selection of questions designed to reveal Dr. Chauvin's experience in experimental parapsychology and his attitudes toward some of the problems in the field. The conversation proceeded as follows:

J. B. R.: Because your work in parapsychology is not all available in English and not all identified with your name, I think we are more in the dark about your earlier writing on the subject and your original approach to this field than we are with anyone else who has done so much. Would you therefore, as it were, introduce yourself to us from the point of view of your original interest and approach and your first steps, both in research and publication on the subject?

R. C.: My first writings in parapsychology were published in your *Journal*; but my approach began four or five years earlier with—I have to admit it—very badly designed experiments. It was the discovery of a French edition of your book *New Frontiers of the Mind* that acquainted me with what had been done elsewhere and what modern parapsychology was like.

J. B. R.: Your research in parapsychology has been exceptionally broad in scope. You have dealt with both ESP and PK, with both human and animal subjects, with both the subjective and the objective conditions of the test, and in your PK research, with both microphysical and macrophysical target objects. From your comparative judgment of these various research efforts what now seems the most promising direction to take, the best area for the continued work you are planning to do?